SECTION II.—GENERAL METEOROLOGY.

A MUCH NEEDED CHANGE OF EMPHASIS IN METEOROLOGICAL RESEARCH.

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Meteorology is generally conceded to be the least advanced of all the physical sciences. While considerable advances have been made in forecasting based on telegraphed information, forecasting from observations at a single station is but little better than has been possible for a thousand years among weatherwise farmers. And not even a definite scientific conception of the possibility of weather control has hitherto existed. These are very important phases of meteorology, weather prediction, and weather control, and it is no wonder that many laboratory physicists who do predict and control should have a contempt for meteorological studies, and especially for those of statistical character. Also, in view of the wonderful developments in laboratory physics, it is natural that many meteorologists should be more or less apologetic in their attitude toward their work. It seems, however, to the author that contempt for statistical studies on the part of the laboratory physicist is wholly unjustifiable, and very certainly no meteorologist should take an apologetic attitude toward his chosen line of work.

One object of this paper is to show that the ordinary point of view of the laboratory physicist is extremely narrow and his disapproval of statistical studies not to be taken very seriously. Another object is to convince the meteorologist that he has been too much imbued with the older point of view of the laboratory physicist, and still another object which is, in fact, the chief object of the paper, is to set forth a new point of view, which is beginning to develop among laboratory physicists, a point of view which may be expected to lead to a very great increase of interest in the use of the statistical method. Indeed it seems probable, in the author's opinion, that statistical studies may come to be as alluring as the classical physics.

The most remarkable thing concerning the older methods in the physical sciences is that every measurement, every laboratory study, is and must be made in a region in equilibrium, in thermal equilibrium more or less nearly complete. The older physics is the study of permanencies,[1] as explained in the following pages. Actual movements in nature, actual happenings, things which do occur, have never been the immediate objects of study by the physicist or chemist. If an actual movement presents a dominant state of permanency, like the motion of the earth in its orbit, this dominant state of permanency is abstracted from the complex movement as a whole and treated by the well-known method of mechanics. If an actual movement presents a state of quasi equilibrium, like the slow saponification of an oil in an alkaline solution, this state of quasi equilibrium is abstracted from the complex movement as a whole (the solution may be violently agitated for example) and studied as a chemical reaction velocity. If we study a steady flow (approximately steady flow) of heat, we arrive, by more or less abstraction, at the quantitative laws of heat conduction and emission. In every case in which quantitative correlations are established in the laboratory the experimental system is brought more or less completely under control to a state of permanency. For example, we take a batch of gas and bottle it up and protect it from outside disturbances by thick layers of cotton or wool, we use a long-stemmed thermometer to measure its temperature, and we change its volume with extreme slowness and make measurements of corresponding temperatures, pressures, and volumes, and it is not surprising that we find a very definite correlation between temperature, pressure, and volume of such a quiet body of gas.[2] Let it be understood that we are not here concerned with the fact that Boyle's law, for example, is not exactly true, but we are concerned with the fact that there is a definite relation between the pressure and volume of a given body of gas at a given temperature. The mathematicians call such a relation a one-to-one correspondence.

All of the classical laws of physics are one-to-one correspondences; they relate always to substances or systems in permanent states; they are approximate only[3] when they refer to quasi permanence, and the essential narrowness of the classical method in physics and chemistry has never been more strikingly pointed out than by Goethe, who, being a poet, could not be expected to recognize the necessity or appreciate the tremendous practical importance of any narrow and prosaic point of view.

> "Wer will was Lebendiges erkennen und beschreiben
> Sucht erst den Geist heraus zu treiben."

Whoever undertakes to study things seeks at the beginning to drive the spirit out. To make any detailed chemical study of wood, even of dead wood, one must crush it and cook it and by dissolution reduce it to one or more homogeneous substances. In studying any complicated chemical phenomenon one must limit oneself to what may be called, in mild derision, the "before" and "after" method,[4] comparing the quiet initial state of a system with its quiet final state, and one makes a discovery if one establishes a relation between associated changes which take place in two systems.[5] •

[1] The reader will be very greatly helped in getting a clear idea of what is here referred to if he will look up a simple discussion of the mechanical notion of force which is given on pages 322-323 of Franklin and McNutt's General Physics, McGraw-Hill Book Co., New York, 1916. This discussion is an exposition of some of the simpler ideas which are set forth in a remarkable article by Sir Joseph Larmor "On the Scope of Mechanical Explanation." The present paper must be kept within readable limits, and the author considers it necessary to supplement the paper by references, which may help definitely and clearly to accomplish his object. The article by Sir Joseph Larmor is very difficult to read, and it would be useless for any reader of moderate mathematical attainments to attempt to read it.

[2] It is a common mistake to think of the classical laws of physics as *inexact* when they are merely *not simple* from the arithmetical or algebraic point of view. The pressure of a body of oxygen at a fixed temperature is not exactly in inverse proportion to the volume, but for each volume there is a perfectly definite pressure, perfectly definite if one has a sufficiently large body of oxygen, if it is sufficiently protected from outside disturbances, and if measurements of volume and pressure are taken so as to refer to the average behavior of the gas over a very long interval of time, as long as a few thousandth's of a second, let us say.

[3] Or when the experimental system is very small or the measurements taken too quickly. The atomic theory suggests that this qualification applies to every kind of one-to-one correspondence in nature and careful observation has verified this suggestion in a few instances.

[4] See outline of boiler test under the heading "Thermodynamics and the atomic theory."

[5] The ideas of energy and entropy have to do wholly with associated changes in separate systems, and however strongly inclined the reader may be to think of energy "as the only real thing in the universe," the author insists that to understand this paper one must adopt the naive point of view that physically real things are simply the things that we see and feel. Precise ideas (mathematical ideas) are tremendously important in that they open the mind for the perception of the simplest evidences of a subject, as Whewell has said, * * * provided the evidences are consistent with the ideas. Accepted ideas close the mind almost completely to contrary evidences. Ideas help tremendously to form our sense of physical things * * * but they inhibit sense as well. This matter is discussed in a very naive manner in the introduction, pages 1-15, of Franklin and McNutt's Mechanics and Heat, Franklin and Charles, Bethlehem, Pa., 1910.

Errors of measurement—Probable errors and probable departures.

It has long been the custom to speak of the probable error of a precise measurement as if perfect precision would be possible if our measuring devices were perfect and free from erratic variations. It is important, however, to recognize two distinct types of erratic error, namely, extrinsic error due to uncontrollable variability of the measuring device or system and intrinsic error due to inherent variability of the thing or system which is being measured. Every physical measurement involves an operation of congruence, a standard of some kind is fitted to or made congruent with successive parts (which parts are thereby judged to be equal parts) of the thing or system which is being measured; and the standard system and the measured system are both subject to erratic variations.

There is, perhaps, no case in which intrinsic error and extrinsic error can be clearly distinguished and separated from each other; but when the errors of one kind are much larger than the errors of the other kind, they can, of course, be recognized. It is proper to speak of the *probable error* of a single measurement when the variations of the measuring device or system are dominant, but one should speak of the *probable departure* of the measured system from a certain expected condition at any time when the "errors" of observation are due chiefly to variability of the thing or system which is being measured. Thus, in measuring the coefficient of sliding friction extrinsic error may be made negligible by making the measurements carefully, but very large "errors" persist. The thing which is being measured is inherently indefinite, and it may at any time depart widely from the most carefully considered expectation.[6]

Hydraulics as illustrating the method of mechanics.

Hydraulics is the study of fluids in motion, and the phenomena of fluid motion are to the careful observer excessively complicated. Even the apparently steady flow of a river through a smooth sandy channel is an endlessly intricate combination of boiling and whirling motion; and a jet of spray from a hydrant or a burst of steam from the safety valve of a locomotive, what is to be said of such things as these? Or let one consider the fitful motion of the wind as indicated by the swaying of trees and the quivering of leaves and as actually visible in driven clouds of dust and smoke, or the sweep of flames in conflagration. These are actual examples of fluid motion and they are indescribably, infinitely,[7] complicated.

The science of hydraulics is based upon ideas which relate to average aspects of fluid motion. Thus the engineer is concerned chiefly with such things as the time required to draw a pail of water from a hydrant, the loss of pressure in a line of pipe between a pump and a fire nozzle, or the force exerted by a water jet upon the buckets of a water wheel. These things are never perfectly steady but they are always subject to perceptible fluctuations of an erratic character, and to think of any one of these effects as definitely quantitative is of course to think of its average character under the given conditions; and the extent to which the science of hydraulics is limited by the consideration of average effects is evident from the following outline of the fundamental idea of simple flow.

When water flows steadily through a pipe or channel the motion is always complicated more or less by continually changing eddies; the water at a given point does not continue to move in a fixed direction at a constant velocity. Nevertheless it is convenient to treat the motion as if the velocity of the water were in a fixed direction and of constant magnitude at each point. Such ideal fluid motion is called simple flow, and to use the idea of simple flow in the study of an actual case of fluid motion is the same thing as to consider the average character of the motion during a fairly long interval of time. Also to make use of the idea of lamellar flow in the study of pipes and channels is to consider the average velocity over the entire section of the pipe or channel.

A gardener is not concerned with the size or shape of a particular drop of rain or whether it falls on the north or south side of a particular clod of earth—all such erratic details are to him of no consequence; but the erratic movements of a fluid are not always unimportant as the experienced seaman and especially the experienced aviator knows too well, and where individual cases of erratic behavior are important the classical method in physics is somewhat limited in its usefulness. Indeed the use of the classical method tends to divert one's attention away from erratic happenings, however important they may be.

Thermodynamics and the atomic theory.

In nearly every branch of physical science there are two more or less distinct methods of attack, namely, (*a*) a method of attack in which the effort is made to develop mechanistic conceptions or models of physical and chemical processes, and (*b*) a method of attack in which the effort is made to correlate phenomena on the basis of measured data; and these two methods stand out in sharpest contrast in the study of heat phenomena. The first method is the application of the *atomic theory* and the second method is called *thermodynamics*.

The atomic theory is used in every branch of physics but to develop the contrast between the atomic theory and thermodynamics we will limit our discussion to the subject of heat (which properly includes the whole of chemistry). Every student of elementary chemistry is familiar with some of the uses of the atomic theory. What happens when carbon burns, for example? Two atoms of oxygen fall upon an atom of carbon and form a molecule of CO_2, which when formed is in violent agitation. Let the reader recall the well-known ideas; there is no need to dwell upon them here. But in the purely thermodynamic method one is not concerned with thermal and chemical actions themselves but with their results. Imagine an engineer squinting into a furnace and making a minute microscopic study of every flicker of flame and of every curl and puff of smoke. It is not done. The important and feasible thing in a boiler test is to study (1) the condition of the supply water from which the steam is made (2) the qualities of air and coal which are to combine in the furnace (3) the pressure and temperature of the steam which is to be produced, and (4) the quality and temperature of the flue gases as they enter the chimney. That is to say, it is important (and feasible) to consider

[6] We are here necessarily anticipating what is referred to later as the postulate of indeterminism.

[7] Everyone concedes the idea of infinity which is based on abstract number—one, two, three, four, and so on ad infinitum—and the idea of infinity which comes from the contemplation of a straight line. But most men are concerned with the humanly significant and more or less persistent phases of the material world; their perception does not penetrate into the substratum of erratic action which underlies every physical happening, and they balk at the suggestion that the phenomena of fluid motion, for example, are infinitely complicated. Surely the abstract idea of infinity is as nothing compared with the awful intimation of infinity that comes from things that are seen and felt.

only the state of things before and after the combustion takes place and the only measurements that are needed (and the only ones that are feasible or even thinkable) are measurements made of substances in approximate thermal equilibrium.

Systematic physics and statistical physics.

Helmholtz has used the term "systematik" to designate the classical methods in physics, which include the method of mechanics and the method of thermodynamics, and also the atomic theory when it is used, as it often is, to help in the establishment of one-to-one correspondences in systems in permanent or quasi-permanent states. Nearly the whole of physical science has been hitherto what Helmholtz called systematik, and the correlations which have been established are, nearly all of them, one-to-one correspondences, many of which are expressible to a fair degree of approximation in terms of very simple analytical functions.

We often extend the classical method in physics to systems which are very far removed from permanent states, but the ideas which are used and the kinds of correlations which are established are all borrowed from, and relate to ideal permanencies. We may, for example, determine to a certain rough degree of approximation how the members of a bridge structure stretch or shorten as a car passes across the bridge; how electromotive force, current strength, and all the changing variables play in the operation of a dynamo; how the pressure and temperature of the steam vary during the successive stages of admission, expansion, and exhaust of a steam engine; and so on. But all these things are accompanied by very perceptible amounts of erratic action. Everything which takes place in this world has associated with it a substratum of complex action which baffles description. Consider, for example, a simple thing like the movement of a train of cars. The engineer is concerned only with certain broad features of what takes place, the amount of coal and water used, the draw-bar pull of the locomotive, and the forward motion of the cars as affected by steepness of grade and the opposing force of friction. But who could describe in detail the rocking and rattling motion of the cars and the whirling and eddying motion of the surrounding air, and who could trace the motion of every particle of dust and smoke? This indescribably complex action we call by the name of *turbulence;* it exists everywhere and in everything that goes forward in this world of ours, and it is never twice alike in detail even when the conditions are what one would consider exactly the same. All of which suggests two postulates concerning turbulence, namely, (*a*) that it is infinitely complicated, and (*b*) that it is essentially erratic in character. Let it be understood, however, that we are not speaking in terms of ordinary ideas or values in making these two statements. It is not a question, for example, as to whether a brakeman loses his hat every time he makes a trip from Albany to Buffalo, but it is a question as to whether his hat is lost every time at identically the same place because of a gust of wind of precisely the same character when he lets go of it in the same way because of a sudden jerk of the train which always occurs at the same place in exactly the same manner, and so on in endless detail of specification, if such specification were possible.

In the motion of a simple mechanism like the sun and planets or in the operation of a simple machine like a dynamo the accompanying erratic action is practically negligible. Thus one does not consider even the tremendous storm movements in the sun in the study of planetary motion, and one does not consider the minute details of the motion which takes place in a lubricated bearing in the study of the operation of a dynamo. In many phenomena, however, erratic action is dominant. Consider, for example, the motion of the water in a brook. This motion presents a fairly definite average character at each point, and a fairly typical rythmic variation from this average exists at each point, but there is an erratic departure from this regular motion which is by no means negligible in magnitude. So it is in the case of the weather. There is a fairly definite average of weather conditions at a place from year to year, and a fairly typical rythmic variation, but there are, as we all know, wide departures from average and from type, departures which for the most part are erratic in character.

Turbulence is characteristic of those physical and chemical changes which are called irreversible or sweeping processes.[8] The most familiar example of such a process is ordinary fire, and, as every one knows, a fire is not dependent upon an external driving cause, but when once started it goes forward spontaneously and with a rush. Tyndall, in referring to the impetuous character of fire, says that it was one of the philosophical difficultues of the eighteenth century. A spark is sufficient to start a conflagration and the effect would seem to be out of all proportion greater than the cause. Herein lay the philosophical difficulty. This difficulty may seem to be the same as that which the biologist faces in thinking of the small beginnings of such a tremendous thing as the chestnut tree blight in the United States. The chance importation of a spore is indeed a small thing, but it is by no means an infinitesimal, whereas, under conceivable conditions a fire can be started by a cause more minute and more nearly insignificant than anything assignable.[9] This possibility of the growth of tremendous consequences out of a cause which has the mathematical character of an infinitesimal is the remarkable thing; and this possibility is not only characteristic of fire, but it is characteristic of impetuous processes in general.

The postulate of indeterminism.[10]

Impetuous processes, such as storm movements of the atmosphere are intimately connected with conditions of

[8] If the reader is not familiar with the fundamental ideas which are related to and involved in the second law of thermodynamics, and it is safe to assume that he is not, he might profitably read pages 153-169 of Franklin and McNutt's General Physics. This is an extremely simple and vividly physical discussion and it will serve to show how erratic action or turbulence is treated in the classical thermodynamics. This has, of course, an important bearing on the subject matter of this paper.

[9] It is not a valid objection to this statement to say that it is not true * * * according to the atomic theory. Because in the first place the atomic theory hardly carries one so far, and in the second place the atomic theory is, after all, only a group of ideas, and as such it can not properly be allowed to determine every tentative idea that one is to entertain.

[10] The postulate of indeterminism is put forth in a remarkable paper by M. J. Boussinesq, entitled "Conciliation du Véritable Déterminisme Mechanique avec l'Existence de la Vie et de la Liberté Morale," Paris, 1878.

M. Boussinesq's point of view is, however, essentially different from the idea which is here set forth. "Scientists are in agreement," says M. Boussinesq, "that physical and chemical laws are reducible in the last analysis to different equations," and, according to M. Boussinesq, indeterminism is linked with the behavior of differential equations near and at their singular points. This is certainly a very important idea, but the idea here set forth is *that physical laws (differential equations) relate always to permanencies or quasi permanencies,* and that no physical law exists (no differential equation has any meaning) in a turbulent system unless such a system presents what an Irishman might call momentary permanencies and what we, with more elegance, call quasi permanancies.

M. Boussinesq's indeterminism comes from singularities in action, as implicitly contained in a differential equation, whereas the indeterminism here referred to comes from a break, as it were, in initial or boundary conditions and the complete failure in applica*tion of any differential equation (except, of course, such* differential equations as express the application of the principle of probability) until the system again comes to complete or quasi equilibrium. Thus the curve which represents the expansion of a gas on the $p\ v$ diagram presents an actual gap, the curve does not exist, when the gas passes through a turbulent condition.

What is stated above as to the distinction between the Boussinesq indeterminism and the indeterminism which is discussed in this paper, namely, that in one case we have a valid differential equation which has singularities and in the other case a break in bounding or initial conditions and an entire failure in application of any differential equation whatever, does not apply, of course, to any purely mechanical system but only to chemical systems.

instability. Indeed, an impetuous process seems always to be the collapse of an unstable state. Let us consider, therefore, two ideal cases where the condition of instability is assumed to be completely established at the start.

(*a*) Imagine a warm layer of air near the ground overlaid with cold air. Such a condition of the atmosphere is unstable, and any disturbance, however minute, may conceivably start a general collapse. Thus a grasshopper in Idaho might conceivably initiate a storm movement which would sweep across the continent and destroy New York City, or a fly in Arizona might initiate a storm movement which would sweep out harmlessly into the Gulf of Mexico. These results are different surely, and the grasshopper and the fly may be of entirely unheard-of varieties, more minute and insignificant than anything assignable. Infinitesimal differences in the earlier stages of an impetuous process may, therefore, lead to finite differences in the final trend of the process.

(*b*) Consider a smooth spherical ball traveling through still air. There certainly is no more reason to expect the ball to jump to the right than to the left. Therefore we may conclude that it will not jump either way. Similarly, a sharp-pointed stick stands in a perfectly vertical position in a perfectly quiet room, and there is no more reason to expect the stick to fall one way than another; therefore the stick will not fall at all. Everyone appreciates the fallacy of this argument as applied to the stick, and the moving ball does, in fact, jump sidewise.

To understand the behavior of the ball, let us think of the ball as standing still and of the air as blowing past it in a steady stream. The air streams past the ball and slides over a body of still air behind the ball; the surface which separates the moving air and still air is called a vortex sheet, and a vortex sheet is unstable. Any cause, however minute, is sufficient to start an eddy or whirl, and once started such an eddy or whirl develops more and more. Such an eddy or whirl means that the air streaming past one side of the ball is thrown inward or outward, and the reaction on the ball pushes the ball sidewise. This effect can be shown by dropping a marble in a deep jar of water. Instead of moving straight downward the marble follows an erratic zigzag path. This effect is familiar to everyone in the sidewise quivering of a stick in a stream of water; and the hissing of a jet of steam is due to the rapid fluttering of the boundary between steam jet and air because of the formation of innumerable eddies.

Whenever the postulate of erratic action is set forth, and the probable departure of a natural phenonenon from the most carefully considered prediction is urged as in the nature of things inevitable, we meet objections from two classes of men, namely, the average man who thinks frankly in terms of human values (the gardener who is not concerned with individual drops of rain) and the classicist in science who idealizes nature in one-to-one correspondences. Surely the classicist says, "if we knew all" the data we could make an unqualified prediction in any case. But, ignoring the hopelessly unscientific attitude of mind of one who can postulate infinite knowledge, let it be understood that to speak of data in physics is to speak of a very narrow and limited kind of thing, for data are conceivable only where measurements can be made or where we have, contrary to Bacon's exhortation, accepted a dream of fancy for a model of the world.

In that branch of mathematical physics which is called statistical mechanics and which includes the atomic theory, we speak of the complection of a system when we wish to refer to the positions and velocities of all the elements or particles of the system. Let us use this word in the statement of the postulate of indeterminism. *The complection of the world to-morrow is not determinate—that is to say, it does not grow out of the complection of the world to-day as a single-valued determinate thing.* This is a postulate which, as it seems, must be accepted as a working hypothesis in the "extra-equilibrium" world, the world of actual happenings, where things never do stand still but go forward by fits and starts impetuously and beyond all control.

The conception of a physical system as an atomic aggregate and reason for steadiness of behavior.

Let us consider a body of gas in a closed vessel. According to the atomic theory such a gas is an aggregate of a vast number of minute things and conditions, no one of which ever exercises a dominant influence upon the behavior of the system. Therefore, according to the principles of probability, the body of gas should present a remarkable steadiness of behavior and this steadiness of behavior should be more and more nearly complete the greater the amount of gas and the longer the time over which the behavior is averaged.

But suppose a state of affairs could be brought about in which a single one of the minute elements or conditions could be made to play a preponderating rôle in the behavior of the gas like the above-mentioned Idaho grasshopper or the Arizona fly. Then the gas would be extremely erratic in its behavior and the kinetic theory of such a gas would have to be something more than a straightforward development of the simple principle of probability to be of any great service. We would be interested in peculiar individual tricks of such a gas, and it is conceivable that these tricks might be infinite in variety. Mere averages, even mere averages qualified by probable departures, would be of little significance. A particular trick of the gas might be dependent upon the sudden dominance of one peculiar element or condition, and this particular trick might only occur once in a thousand years. Once in a thousand years, and we might be concerned with the behavior of the gas tomorrow. The only help in this case would be to study individual tricks with great care and patience and thus be able to recognize a particular trick in its incipiency and anticipate more or less accurately its future details of development. Classification studies of the most exhaustive sort, the recognition of certain type tricks and the correlation by probability methods of the details of development and extent of departures of an individual from normal or type should be made.

Imagine the gas, for example, to be slowly changing through a long series of equilibrium conditions, and imagine each of these equilibrium conditions to be in danger of breaking, as it were. Thus the containing vessel might be imagined to change its shape and temperature in a most complicated manner, growing thin at certain places and in danger of rupture and the gas might stream from one portion of the container to another and develop unstable states of motion which might break as a vortex sheet is known to break. This highly fictitious example is given to lead up to a fairly adequate statement of a very important fact as follows: *Any adequate statistical study of the behavior of a system which depends upon highly heterogeneous erratic elements must use exhaustive classification studies.* To be content with averages and departures derived from inadequately classified data would in such a case be as ineffective as to ignore the existence of species in the study of living forms.

Meteorology.

Meteorology has for many years been, one might almost say, the only branch of physics concerned with actual happenings, and the statistical methods which have been used in the past have been too much of the kind that are applicable to a homogenous aggregate of erratic elements. Statistical meteorological studies have in most cases involved the leveling process of simple averaging, whereas the conditions would seem to require classification, classification ten thousand times more exhaustive than any hitherto made. Of course there is some interest attached to the average in meteorology because systematic differences and rhythmic changes which are extremely important are discovered in this way, but even such things would be more easily detected if the averaging process were based upon a much more exhaustive scheme of classification. In thinking of emigrating to Mesopotamia after the war, the writer has wished to know what the average weather conditions there might be for a lifetime or two, but as a resident of Boston, he is chiefly concerned with the variations of Boston's weather. Will the late frosts next spring deprive him of apples? Will winter's cold make him wish he were in Florida? And if he should be driven to Florida what weather welcome would he get there? It is certain that the most important phase of meteorology is to deal with particulars and not with averages, and its ultimate aim is weather control.

Saying very little, therefore, of the need of researches of the classical kind in meteorology, for everyone recognizes this need, let us point out what seems to be most urgently needed in the most difficult phases of the subject and we make the statement brief in the hope that it will be interpreted with the help of what we have said concerning the new point of view that is developing among physicists.

Three fairly distinct objects are to be attained in the analysis of weather observations, namely:

(*a*) The determination of systematic variations in time and place. This object has long been recognized by meteorologists.

(*b*) The elaborate classification of individual storm movements with respect to a great number of measurable or specifiable characteristics, and the establishment of statistical coefficients of correlation between the characteristics of a given type of storm on successive days so that weather predictions can be made and qualified, as they should be, by probable departures. This object has, of course, been recognized by meteorologists but we believe that classification studies should be very greatly extended.[11] Effective schemes of classification can only be developed under the stimulus of intensive study of actual weather conditions (weather maps, let us say). This sort of study might properly engage the whole time of a large staff of men, and probably the observational work and instrumental equipment of the Weather Bureau would have to be altered in response to the clearly conceived demands for new kinds of data.

(*c*) The intensive study of weather conditions should lead to a clear recognition of critical conditions in a given storm movement (conditions of static or dynamic instability) and make it possible to devise means for controlling the storm movement by the suitable expenditure of very moderate amounts of energy at the critical time and place. Anyone who has seen an old-fashioned prairie fire brought under control by carefully considered backfiring, and who recognizes the meanings of static and dynamic instability in their influence on a complex physical system like the atmosphere, will accept this idea of weather control as a legitimate conception, to say the least. Whether it can ever be actually realized, however, is another thing; but it seems well worth the attention of the meteorologist. Although every atmospheric movement may, perhaps, be properly thought of as the collapse of an unstable state, it is probable that such collapse is already well under way in the earliest stages [12] of every movement so that extremely critical states may never develop. Therefore the energy required to control a storm movement might always be considerable in amount from the human point of view, although extremely small as compared with the total energy of the storm movement itself.

[11] Some of the more recent classification studies are as follows: Bowie, E. H., & Weightman, R. H.: Types of Storms of the United States and their Average Movements, M. W. R. Suppl. 1, 1914; Types of Anticyclones of the United States and their Average Movements, M. W. R. Suppl. 4, 1917.

Henry, A. J., Bowie, E. H., Cox, H. J., & Frankenfield, H. C.: Weather Forecasting in the United States, W. B. Pub. No. 583, 1916.

[12] The discussion of simple sweeps and of steady sweeps on pages 154–155 of Franklin and MacNutt's General Physics will help to make this clear.

DYNAMIC HEATING OF AIR AS A CAUSE OF HOT VOLCANIC BLASTS.

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Probably there is no field so virgin or inviting for scientific marshaling as that part of the meteorological domain which has to do with atmospheric phenomena caused by, and following, volcanic eruption. Its ramifications reach from the vapor condensation and electrical effects of the storm to the dynamic record of the destruction of things, animate and inanimate, that fall before it. At the eruption of Mont Pelé 30,000 human beings were killed in an instant under most appalling and marvelous circumstances, yet not a single autopsy was performed and no published account can be found that assigns a sure explanation of the cause of death.

In a previous paper [1] the writer has quoted at length eminent authorities who set for themselves the question, "What causes the deaths?", and all give practically the same answer, "The hot blast, bearing sand and steam from the volcano". This is corroborated by the learned Viennese geologist, Prof. E. Suess, and by B. Koto,[2] of Japan, who refer to the "hot vapors and steam cloud of Mont Pelé."

It will not be the function of this paper to inquire into the ordinary smoke, vapor, and electrical effects of the eruption. They are probably of the usual nature of storm phenomena, greatly intensified by the magnitude of the action, which involves great differences in temperature, with unusual volumes of water and volcanic matter. Our task will be an inquiry into those blasts which, with swift and stifling stroke, rend, burst, blister, ignite, and upheave all that oppose them till their fury is dissipated by distance and their ardor cooled by mixture.

Volcanoes are not laboratories. They can neither be selected in advance with even approximate certainty, nor can safe stations for apparatus be chosen. After the experiment, physical and biological changes of enormous magnitude must be read in terms of analogy from our experience.

Just after the Martinique disaster it was related to the writer that bodies had been found apparently unharmed and untouched except for blisters and burns and that these blisters were under clothing, at times as frail as lace, that showed no trace of fire or scorch, nor were there contiguous evidences of fire. These people had

[1] Scientific American Supplement, May 25, 1918, 85: 334–336.

[2] Tokio Imp. Univ., Coll. of Sc. Jour., 38, 1916–17, art. 3. B. Koto, p. 71